

to the greatest palmation of the horns, than is the case in the other three specimens. The explanation of this discrepancy is very simple. The former represents (as its own characters and a comparison of it with the remainder of the fragments with the species found at Clacton readily prove) a young animal, probably a buck of four years of age, whilst the other figures represent the horns of adult animals. In the immature Clacton Deer

the force expended in producing the abnormal tyne *d* well-nigh exhausted the supply at the command of a system fully occupied with the production of things more needful, namely, materials for the vigorous increase and consolidation of flesh and bone. Hence the long, attenuated palm, which probably ended very much in the manner in which Mr. Boyd Dawkins has restored it. Analogous instances of excess of growth in one direction,

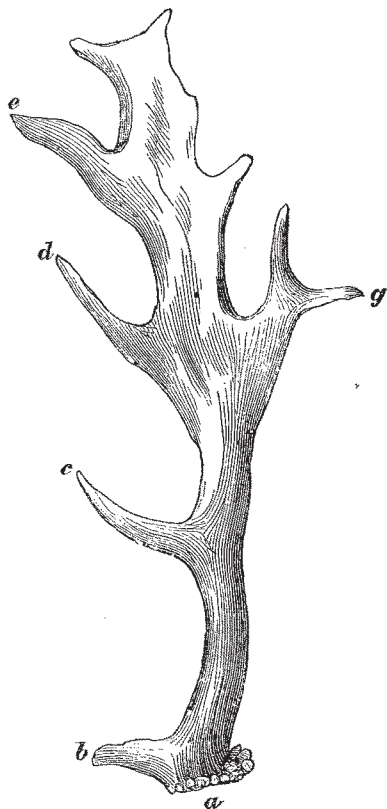


FIG. 3.—Right Horn of Wild Sardinian Fallow Deer.

causing a corresponding defect in another, may be seen in all large collections of deer's horns; indeed, in my own collection I find the horn of a young fallow buck, in which the characters specially alluded to in the type of *Cervus brownii* are shown in a still more marked degree.

These facts appear to me fully to justify the rejection of *Cervus brownii* as a species distinct from *Cervus dama*, and therefore to warrant the belief in the existence of this species in England during Pleistocene times. Whether the Fallow Deer became extinct in

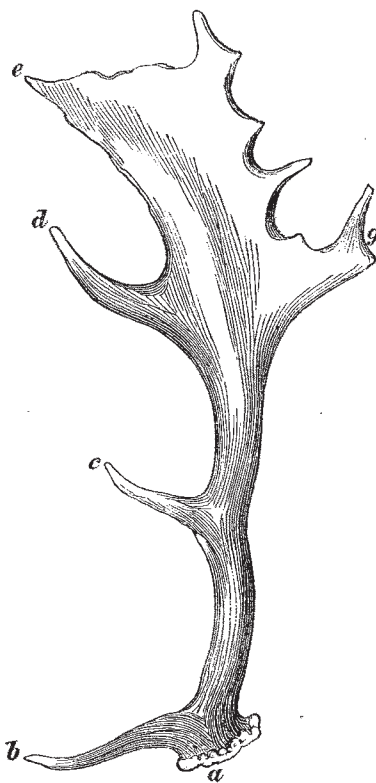


FIG. 4.—Right Horn of Park Fallow Deer.

Northern Europe before the advent of Prehistoric man, or whether it continued to exist in these islands even at the commencement of the Roman occupation, are questions which strike me as altogether beside that of the truth of the "ancient belief" to which Mr. Boyd Dawkins shows such firm allegiance. In either case, the species may have been reintroduced by the Romans, a people whose magnificently lavish expenditure upon luxury and pleasure despised bounds.

VICTOR BROOKE

#### HELMHOLTZ ON THE USE AND ABUSE OF THE DEDUCTIVE METHOD IN PHYSICAL SCIENCE\*

WE have still to speak of his attack on the authors of this book with regard to the emission theory of light. They say that such a theory is not to be justified unless a light-corpuscle has been actually seen and investigated. In this demand Mr. Zöllner detects "an impossibility which is not simply physical, but even logical, and which it is easy to expose. In fact, if the sensation of light is produced only when the corpuscles come in contact with our nerves, it is obviously impossible to have any ocular perception of such a corpuscle before it has touched or affected our nerves of sight." And then this remark is followed by declamation about gross blunders

\* Concluded from p. 151.

in logic, absolute nonsense, and so on. And, in fact, there is absolute nonsense here; only the nonsense does not lie in what the English writers have said, but in the interpretation which their opponent has put upon their words. Does a man who believes himself so superior to his antagonists in the firmness of his grasp of the principles of the theory of knowledge, still need to have it explained to him that to see an object means, according to the emission theory, to receive in the eye, and so to feel, the corpuscles of light that rebound from the object in question? But, this being so, there is no logical impossibility, and nothing inconsistent with the premises of the theory, in the supposition that a light-corpuscle at rest—and the corpuscles are at rest as soon as they are absorbed by dark bodies—may throw off other corpuscles that impinge on it, and so may become for these a centre of radiation, which will be visible as the radiant point. Whether such

a process can be brought under observation, and how this is to be effected, are, of course, questions which, on the argument of the English authors, fall to be answered by those who undertake the direct proof of the existence of the corpuscles. And whatever opinion one may form of the stringency and fitness of this demand, it involves no logical contradiction, which is the very point on which the argument must turn if Mr. Zöllner is to make good his case.

I will mention one other objection of similar scientific value, because it refers to Sir W. Thomson, though not to a passage of this book. The point in question is whether it is possible for organic germs to be present in meteoric stones, and so to be conveyed to worlds which have become cool. In his introductory address to the British Association at Edinburgh, in the autumn of 1871, Sir W. Thomson characterised this view as "not unscientific." Here, too, if an error has been committed, I must profess myself a sharer in it. I had, in fact, indicated the same view as a possible explanation of the transmission of organisms through interstellar spaces at a somewhat earlier date than Sir W. Thomson—in a lecture which was delivered at Heidelberg and at Cologne in the spring of the same year, but is still unpublished. If anyone chooses to regard this hypothesis as highly or even as extremely improbable, I have nothing to object. But if failure attends all our efforts to obtain a generation of organisms from lifeless matter, it seems to me a thoroughly correct scientific procedure to inquire whether there has ever been an origination of life, or whether it is not as old as matter, and whether its germs, borne from one world to another, have not been developed wherever they have found a favourable soil. The physical reasons alleged by Mr. Zöllner against the view in question are of very little weight. He points to the heating of the meteoric stones, and adds (p. 26): "Thus, even if we suppose that when the parent body was shattered, the meteoric stone covered with organisms escaped with a whole skin, and did not share the general rise of temperature, it was still necessary for it to pass through the terrestrial atmosphere before it could discharge its organisms to people the earth."

Now, in the first place, we know from oft-repeated observations that of the larger meteoric stones only the very surface is heated in passing through the atmosphere, the inner portions remaining cold, or even very cold. All germs, therefore, that happened to be in cracks of the stone would be protected from combustion in our atmosphere. But even germs lying on the surface would doubtless, when they entered the very highest and most attenuated strata of the earth's atmosphere, be blown away by the powerful current of the air long before the stone reached the denser parts of the gaseous mass, where the compression becomes great enough to generate considerable warmth. And on the other hand, with regard to the collision of two worlds as assumed by Thomson, the first consequences of such an event would be violent mechanical motions, while heat would be generated only in proportion as these motions were destroyed by friction. We do not know if this would last for hours, or days, or weeks. The fragments, therefore, projected in the first instant with planetary velocity might escape without any development of heat. I do not even think it impossible that a stone, or swarm of stones, flying through lofty strata of the atmosphere of a world might catch up and sweep along a quantity of air containing unburnt germs.

I have already said that I should not yet be willing to put forth all these possibilities as probabilities. They are only questions the existence and range of which must be kept in view, so that if opportunity offers they may be solved by actual observation or by inferences from such.

Mr. Zöllner then ascends to the two following propositions:—

"That scientific investigators in the present day

attach such extraordinary importance to *inductive proof* of *generatio æquivoca*, is the clearest mark of their lack of familiarity with the first principles of the theory of knowing."

And again:—

"In like manner the hypothesis of *generatio æquivoca* expresses . . . nothing else than the condition for the conceivableness of nature in accordance with the law of causality."

Here we have the genuine metaphysician. In view of a presumed necessity of thought, he looks down with an air of superiority on those who labour to investigate the facts. Has it already been forgotten how much mischief this procedure wrought in earlier stages of the development of the sciences? And what is the logical basis of this lofty standpoint? The correct alternative is clearly this:—

"Either organic life began to exist at some particular time, or it has existed from all eternity."

Mr. Zöllner simply omits the second of these alternatives, or thinks that he has set it aside by a passing reference brought in shortly before to certain physical considerations which are not in the least decisive. Accordingly his conclusion, which affirms the first of the alternatives above stated, is either not proved at all, or proved only by the aid of a minor resting on physical arguments (and, for that matter, inadequate physical arguments). The conclusion, therefore, is not in any sense, as Mr. Zöllner believes, a proposition of logical necessity, but at most an uncertain inference from physical considerations.

This is what Mr. Zöllner has to object to the authors of this handbook in the sphere of scientific questions.\* Mr. Zöllner's book contains a great number of other accusations of precisely the same value directed against other scientific investigators, with the same confidence in his own infallibility and the same rash haste in passing judgment on the intellectual and moral qualities of his antagonist. Another opportunity will present itself for the discussion of another part of these cases. If I may draw by anticipation a moral interesting to us in the present connection, I would say that no theoretical arguments can present to the attentive and judicious reader a stronger and more eloquent justification of the strict discipline of the inductive method, the loyal adhesion to facts which has made science great, than is supplied by the practical example of the consequences of the opposite, would-be deductive, or speculative method given in Zöllner's book; and this all the more that Mr. Zöllner is beyond question a man of talent and knowledge, who did most promising work before he fell into metaphysics, and even now shows acuteness and the faculty of invention whenever he is limited to the field of the actual, *e.g.* in the construction of optical instruments and the devising of optical methods.

#### NEW ZEALAND PLANTS SUITABLE FOR PAPER-MAKING

THE utilisation of waste materials for paper-making is a subject upon which a great deal has been said and still remains to be said and done. In every country waste vegetable matter which contains fibre in anything like suitable proportions is sure to attract much attention. The subject has been handled in various works, directly or indirectly, in this country as well as on the Continent; and with regard to Australian plants suitable for paper-making, Baron Mueller, of Melbourne, issued a lengthy treatise in connection with a series of specimens of paper actually made from the plants enumerated and exhibited in the Paris Exhibition of 1867. We have now before us a paper by Mr. T. Kirk, F.L.S., of Wellington, on

\* In the region of personal questions, and with reference to the claim of priority as to the principles of spectral analysis made by Sir W. Thomson for Mr. Stokes against Mr. Kirchhoff, I must side with the latter, fully agreeing with the reasons which he has himself brought forward.